

in, and a strongly corrosive thin discharge, with which much epithelium is thrown off. This increases, after a few hours, to a painful inflammation of the mucous membrane and of the outside of the nose, and excites fever with severe headache and great depression, if the patient cannot withdraw himself from the heat and the sunshine. In a cool room, however, these symptoms vanish as quickly as they come on, and there then only remains for a few days a lessened discharge and soreness, as if caused by the loss of epithelium. I remark, by the way, that in all my other years I had very little tendency to catarrh or catching cold, while the hay fever has never failed during the twenty-one years of which I have spoken, and has never attacked me earlier or later in the year than the times named. The condition is extremely troublesome, and increases, if one is obliged to be much exposed to the sun, to an excessively severe malady.

"The curious dependence of the disease on the season of the year suggested to me the thought that organisms might be the origin of the mischief. In examining the secretions I regularly found, in the last five years, certain vibrio-like bodies in it, which at other times I could not observe in my nasal secretion. . . . They are very small, and can only be recognised with the immersion-lens of a very good Hartnack's microscope. It is characteristic of the common isolated single joints that they contain four nuclei in a row, of which two pairs are more closely united. The length of the joints is 0.004 millimetre. Upon the warm objective-stage they move with moderate activity, partly in mere vibration, partly shooting backwards and forwards in the direction of their long axis; in lower temperatures they are very inactive. Occasionally one finds them arranged in rows upon each other, or in branching series. Observed some days in the moist chamber, they vegetated again, and appeared somewhat larger and more conspicuous than immediately after their excretion. It is to be noted that only that kind of secretion contains them which is expelled by violent sneezings; that which drops slowly does not contain any. They stick tenaciously enough in the lower cavities and recesses of the nose.

"When I saw your first notice respecting the poisonous action of quinine upon infusoria, I determined at once to make an experiment with that substance, thinking that these vibronic bodies, even if they did not cause the whole illness, still could render it much more unpleasant through their movements and the decompositions caused by them. For that reason I made a neutral solution of sulphate of quinine, which did not contain much of the salt (1:800), but still was effective enough, and caused moderate irritation on the mucous membrane of the nose. I then lay flat on my back, keeping my head very low, and poured with a pipette about four cubic centimetres into both nostrils. Then I turned my head about in order to let the liquid flow in all directions.

"The desired effect was obtained immediately, and remained for some hours; I could expose myself to the sun without fits of sneezing and the other disagreeable symptoms coming on. It was sufficient to repeat the treatment three times a day, even under the most unfavourable circumstances, in order to keep myself quite free.* There were then no such vibrios in the secretion. If I only go out in the evening, it suffices to inject the quinine once a day, just before going. After continuing this treatment for some days the symptoms disappear completely, but if I leave off they return till towards the end of June.

"My first experiments with quinine date from the summer of 1867; this year (1868) I began at once as soon as the first traces of the illness appeared, and I have thus been able to stop its development completely.

"I have hesitated as yet in publishing the matter, because I have found no other patient* on whom I could try the experiment. There is, it seems to me, no doubt considering the extraordinary regularity in the recurrence and course of the illness, that quinine had here a most quick and decided effect. And this again makes my hypothesis very probable, that the vibrios, even if being no specific form but a very frequent one, are at least the cause of the rapid increase of the symptoms in warm air, as heat excites them to lively action."

I should be very glad if the above lines would induce medical men in England—the haunt of hay fever—to test the observation of Helmholtz. To most patients the application with the pipette may be too difficult or impossible; I have therefore already suggested the use of Weber's very simple but effective nose-douche. Also it will be advisable to apply the solution of quinine tepid. It can, further, not be repeated often enough that quinine is frequently adulterated, especially with cinchonia, the action of which is much less to be depended upon.

Dr. Frickhöfer, of Schwalbach, has communicated to me a second case in which hay fever was cured by local application of quinine (Cf. Virchow's *Archiv* (1870), vol. li. p. 176). Prof. Busch, of Bonn, authorises me to say that he succeeded in two cases of "catarrhus æstivus" by the same method: a third patient was obliged to abstain from the use of quinine, as it produced an unbearable irritation of the sensible nerves of the nose. In the autumn of 1872 Helmholtz told me that his fever was quite cured, and that in the meantime two other patients had, by his advice, tried this method, and with the same success.

THE COMING TRANSIT OF VENUS †

IV.

IT has already been pointed out how unsatisfactory in some respects were the results of the observations made in 1761. Those of the year 1769 were more successful, but the discrepancies of different observers still threw a doubt on the result. After Encke had discussed with all possible care the observations made upon these two occasions, ‡ doubts were still raised as to the correctness of the value thus found for the solar parallax. The reasons of these doubts were manifold. In the first place in order to get any value whatever of the solar parallax, Encke had been forced to assume that enormous errors had been committed by some of the observers; and again, all the other methods of which we have spoken were found to give a tolerably accordant value of the solar parallax, but values that differed considerably from Encke's determination.

It was with no small satisfaction then, that astronomers learnt that M. Powalky in 1864 had deduced a sensibly greater value for the solar parallax, by using more accurate values for the longitudes of the places of observation.

But Mr. C. J. Stone, now her Majesty's astronomer at the Cape of Good Hope, has lately re-discussed these observations. § He finds that when the remarks of the observers are rightly interpreted, all the observations agree without any extravagant errors of observations; and moreover, the value of the solar parallax thus deduced agrees with the values found by other means. Mr. Stone deserves the thanks of the scientific world for having convinced them that this method, which at one time was falling into disrepute, may really be rendered very trustworthy.

The result of Encke's determination was that the mean

* Helmholtz, now Professor of Physics at the University of Berlin, is although M.D., no medical practitioner.—B.

† Continued from p. 14.

‡ *Berlin Abhandlungen*, 1835, pp. 235-310.

§ Monthly Notices of the R.A.S., xxviii., p. 155.

* There is no foundation for the objection that syringing the nose could not cure the asthma which accompanies hay fever; for this asthma is only the reflex effect arising from the irritation of the nose.—B.

distance of the sun from the earth is about 95 millions of miles. It now appears that the true distance is somewhere about $91\frac{1}{2}$ millions of miles. The annexed table gives the values of the sun's parallax and distance as determined by different methods.

Method.	Parallax.	Dist. of sun in miles.	Computer.
Transit of Venus * . .	8''91	91,580,000	Stone
Opposition of Mars † .	8''943	91,240,000	Stone
Lunar Theory ‡ . . .	8''916	91,520,000	Hansen
Lunar Theory § . . .	8''850	92,200,000	Stone
Planetary Theory . .	8''859	92,110,000	Leverrier
Jupiter's Satellites and velocity of light ¶ . .	8''86	92,100,000	Foucault
Constant of Aberration and velocity of light **	8''86	92,100,000	Cornu

The uncertainty of observation which Mr. Stone aimed at clearing away is one of a very curious optical character. It is found that Venus at the time when she has almost completely entered within the sun's disc does not retain her round aspect, but becomes pear-shaped, or at least connected with the sun's limb by a "black drop" or "ligament." This ligament sometimes appears simply as a fine black thread connecting the planet with the limb of the sun. One observer in 1769 saw a number of black cones shooting out to the sun's edge in a fluctuating manner.

Many of these phenomena were doubtless due to bad definition of the telescope employed, or to the instability of its mounting. But the existence of a "black drop" even under the most favourable circumstances cannot be doubted; it was well observed in the case of a transit of Mercury that occurred in 1868.†† If the planet be entering upon the solar disc, the first phase occurs when the edges of the sun and planet seem to be in contact. The second phase occurs at the instant when the "black drop" breaks off and a flood of light sweeps in between the planet and the sun. This occurs very suddenly, and has been supposed to indicate the true time of actual contact.

By referring to the *Philosophical Transactions* of 1769-70, a large number of descriptions of the phenomenon may be read. Some of the appearances are shown in Fig. 14, they are copied from the originals by Bevis, Hirst, Bayley, and Mayer, respectively—Prof. Grant states that the last one bears a resemblance to the appearance of Mercury as seen during its transit in 1868 from the Glasgow Observatory, the sun being near the horizon.

In the case of that transit of Mercury, studied by six experienced observers at Greenwich Observatory, two curious facts appear. Firstly, the times of contact as determined by different observers vary to the extent of $13\frac{1}{2}$ seconds. And secondly, the shape of the planet varied considerably with different observers.

Mr. Stone having noticed a confusion in the language of the astronomers of the last century as to which of the two phases was observed, carefully re-studied their words; and by supposing the two phases to be separated by a constant interval of time, he utilised both kinds of observation. This constant interval of time was deduced from all the observations, and found to be about 17 seconds. In this manner he arrived at the more accurate value of the sun's parallax.

It has been asserted that astronomers claim undue credit for the accuracy of their measurements, since Encke made an error of three or four millions of miles in the calculation of the sun's distance. This is not so. A chemist may be able to weigh many substances with

an error of $\frac{1}{100}$ per cent. or less; but if the substance to be weighed be only $\frac{1}{100}$ of a milligramme, he might have a larger percentage error. When we consider how extremely small an angle the solar parallax is, it is astonishing to find so great a concordance between the results of different methods.

As to the cause of the phenomenon of the "black drop," Lalande ascribed it to irradiation. Irradiation is that curious phenomenon in virtue of which a star, or any bright object, appears larger than it really is. If a thin platinum wire be intensely heated by the passage of an electric current, it seems, to a person distant about fifty feet, to be as thick as a pencil. In this way the sun's diameter seems to be increased. The sun's light also encroaches upon the disc of the planet and makes it seem to be smaller than it really is. But when Venus and the sun have their edges almost in contact, as shown by the dotted line in Fig. 15, then there is no light at that point which can encroach; hence we see at this point the "black drop" to which allusion has been made.

Father Hell, one of the observers in 1769, ascribed the phenomenon of the "black drop" to the sensible size which an illuminated surface must have before it can be visible. There is probably some truth in each of these suppositions.

As to the cause of irradiation, it is difficult to speak with certainty. It is probably caused in part by the telescope and in part by the eye. Great confusion has been introduced by persons neglecting to separate two perfectly distinct phenomena. True irradiation is only observed with a powerful light. With less illumination similar results may be seen, but they are of a different nature, and are produced between the formation of an image on the retina and its reception by the brain. In accordance with the customary nomenclature, this error of vision may be called the *mental aberration* of the eye. It is a perfectly definite phenomenon capable of accurate investigation, and M. Plateau has made measurements of the mental aberration of his own and his friends' eyes.* True irradiation may be caused either wholly or in part, by the spherical aberration or the chromatic aberration of the eye, or by diffraction, or by a spreading of the excitement of the nerves of the retina, which gives rise to the sensation of vision over a sensible space. In a telescope it is probably chiefly due to diffraction.

The success or failure of all observations of contact in the coming transit will to a great extent depend upon our knowledge of the nature of this appearance. For this reason numerous experiments have been made with the object of gaining information upon the question. The Russians, Germans, Americans, and English have all mounted artificial transits of Venus for the practice of observers. The arrangement adopted by the Astronomer Royal consists essentially of a metal disc with two arcs of circles drawn upon it to represent the sun's edge with the metal between them cut away. Behind these there passes a glass plate with a circle of metal to represent Venus let into it flush with its surface. The glass plate is moved by clock-work so that the different phenomena are observed in succession exactly as they will be seen in the true transit. As the artificial planet passes in succession the two arcs representing the sun's edge, the phenomena of ingress and egress are successively observed. Before contact takes place, the sun has two cusps at the point of contact where Venus is touching the edge of the sun. The distance between the points of these cusps rapidly diminishes, the space between them being intensely black. They suddenly meet. But between the planet and the sun's edge a light shade is still seen which lasts several seconds before the planet appears completely detached. If instead of watching the meeting of the cusps, the part between them be studied, a sudden diminution of intensity of the blackness is seen

* Monthly Notices, xxviii., 255.

† Ibid. xxiv., 8.

‡ Comptes Rendus, July 22, 1872.

§ Ibid. 1873. p. 341.

† Ibid. xxiii., 183.

‡ Ibid. xxvii., 271.

¶ Ibid. 1862, p. 502.

†† Monthly Notices, xxix., p. 17, &c.

* Nouv. Mém. de l'Acad. Royal de Bruxelles, t. xi. p. i, &c.

about a second before the meeting of the cusps. The diminution of brightness is very sudden, and this is the phenomenon to be chiefly attended to in the actual observation. It occurs almost exactly at the moment of true contact, though the "black drop" does not disappear until some seconds later. It is of the utmost importance that the nature of these different phenomena should be carefully studied by all the observers. And at the present time experiments are being made with a view of determining the personal equation of each of the observers on the British expeditions.

But the actual observation will be rendered more difficult for various reasons. Firstly, the enormous extent of atmosphere which the rays of light must penetrate before reaching the telescope will destroy the definition to a large extent. Secondly, the existence of an atmosphere around the planet Venus may materially affect the nature of the phenomenon.

In any case there is little doubt that as many of the observers as possible of all countries should describe, as accurately as can be done, the exact appearances which are noticed at successive stages of the ingress and egress respectively. Comparisons being also made between different observers and between different telescopes, it will be possible to reduce the observation of any phase which may chance to be caught in the actual observation to the true time of contact. From observations with the Model Transit of Venus made at Greenwich, the following facts appear :—

1. It requires considerable experience for an observer to appreciate all the definite changes of appearance which occur.

2. When two observers describe a particular phase which they see, and determine to observe this phase together, the times recorded by each are generally accordant within a fraction of a second.

3. The successive phases of an ingress or egress appear to follow each other sometimes rapidly, at other times gradually; so that in some cases all the phenomena are observed within three seconds, on other occasions the same series of phases is completed in ten seconds.

4. The time at which any particular phase is observed varies very slightly with the aperture of the telescope. When a telescope of good definition is employed, the time of any phase at ingress is earlier than with an instrument of less perfect definition.

In the case of the observations of last century, it is easy to see how observers quite unprepared by previous observations as to the nature of the appearances they were about to witness were sometimes inconsistent with each other. In fact, without preliminary practice, and with bad definition, observers might vary even with a Model Transit of Venus by as much as 15 seconds. But, knowing what they are to observe, they would differ under no circumstances by more than about 2 seconds. Hence it is probable that in the actual transit, if the definition be good, the observation *may* be accurate to within one second; but if the circumstances be not very favourable, they may differ to an extent of fully three seconds, even after considerable practice with the model. These estimates serve to give us some idea of the accuracy with which we may hope to have the observations made; and it is probable, from the care which has been taken to multiply the number of observers at each station, that each pair of observations of contact will give us a determination of the parallax of the sun true to about $\frac{1}{2}$ per cent.

In the observations of contact, however, a great deal depends upon the experience of the observer; and it is fortunate that the idea originally thrown out by M. Janssen, and the mechanical execution of which has since been so ably carried out, will indelibly record the progress of the phenomenon and serve as a check to the observers.

By the aid of this method photographs of particular sun-spots have already been taken with great success at intervals of one second during one minute of time. Each of these sixty photographs is perfect in itself, and would admit of very perfect measurements. Hence there is every reason to believe that in this manner an independent and very valuable observation of the true time of contact will be made at each station where a photo-heliograph is situated.

The observations by means of photography during the progress of the transit have few difficulties to contend with. Their value will be largely increased by the fact that the actual measurements will be made afterwards when the observer cannot be carried away by the excitement of the moment. But even in this class of observation there are difficulties which must be carefully considered. It is found that if a sensitised plate be over-exposed, the image of the sun is considerably enlarged. This is due to *photographic irradiation*. It has been found by Lord Lindsay and Mr. A. C. Ranyard to be mainly due to the reflection of light from the back of the glass plate.* It can be almost entirely avoided by wetting the back of the plate, and placing black paper against it. There will still be probably a slight enlargement of the sun's diameter. This will not affect the relative positions of the centres of the sun and Venus; but it will render it extremely difficult to determine the unit of measurement.

There are two ways of applying the photographic method. The first is the same as the heliometric method. For this purpose it is necessary to have one station in the north and another in the south. By the other method we do not determine the least distance between the sun and planet, but the actual position of the planet at each observation. In other words, we determine the distance of Venus's centre from the sun's centre, and also the angular distance measured from the north point of the sun. To do this we must have in the focus of the photo-heliograph a fine thread to indicate the direction of the meridian in the photograph; or in the American method we must have a thread suspended vertically which shall indicate the vertical direction in the solar photograph. The arrangements of the American method, as set up by Lord Lindsay at Dunn Echt, are shown in Fig. 16. The siderostat, lens, and hut, are all shown in position.

The value of the different methods has been well discussed by De la Rue,† Tennant,‡ and Proctor.§ The method which takes into account the *actual* position of the planet on the sun is the more accurate, but it requires that the fiducial lines, or lines of reference, shall be exactly represented in the photographs. Mr. De la Rue says that this can be done to within one minute of space.

Besides photographic irradiation, however, there is a very important difficulty which enters into both the photographic and heliometric methods. This is due to the refraction of our atmosphere. Everyone knows the distorted forms which the sun assumes at the time of sunset. In our own climate these appearances are seldom seen on account of clouds and the haziness of the atmosphere. But even from a high mountain, or from any position which allows the form of the sun to be accurately seen up to the time of sunset, its shape may be noticed to be either square, elliptical, or pear-shaped, according to the circumstances of the atmosphere. Now at the most favourable points of observation the sun will be comparatively near to the horizon. Consequently its form will vary with the temperature of the air and with atmospheric disturbances. With our feeble knowledge of the laws of refraction it will be a matter of some difficulty to determine with accuracy the distance at different times between the centres of the sun and Venus.

* Monthly Notices of the R.A.S. 1872, p. 313.

† *Ibid.* xxix., 48 and 282.

‡ *Ibid.* xxx., 62.

§ *Ibid.* 280.

The same remarks apply to the heliometric method. But with stations chosen where the sun is not too low, we

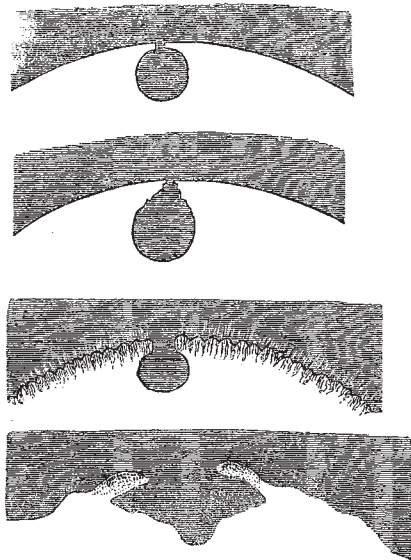


FIG. 14.—The "black drop" as observed in 1769.

may expect accurate results. The value of a heliometer over other instruments designed for measuring small

angles consists in this, that by it we can measure angles as large as the sun's diameter. It is expected by observers with this method that an observation will be made each time with an accuracy comparable with that of an observation of the time of contact. In this case the heliometric method will give valuable results. For the

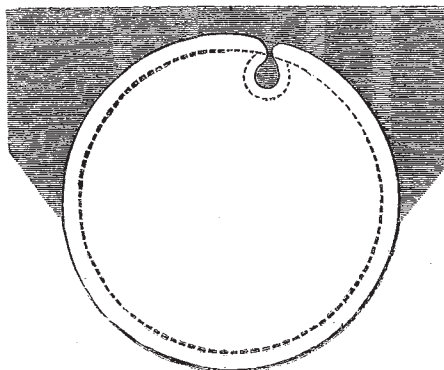


FIG. 15.

same reasons observations made by means of a double-image micrometer of the distance between the limbs of the sun and Venus near the time of contact will be as accurate as an observation of the contact itself.

The last difficulty which we shall mention in connection with this kind of observation is due to atmospheric

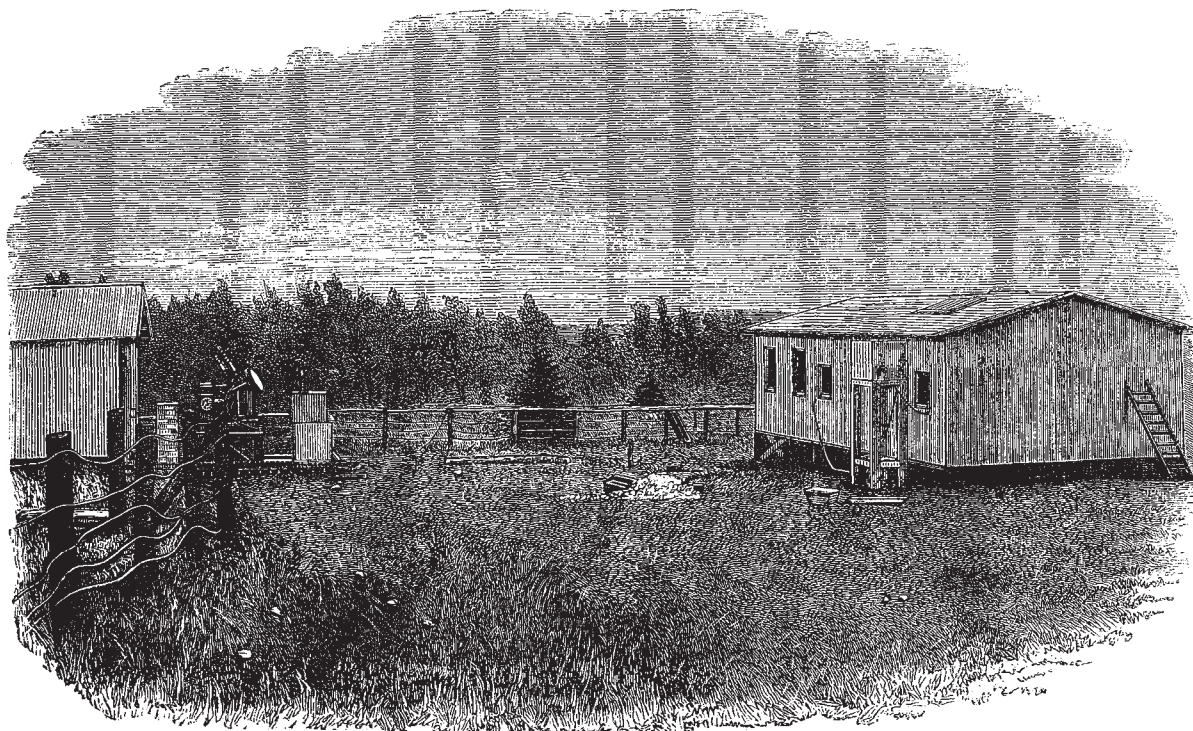


FIG. 16.—Lord Lindsay's Photographic Arrangements as set up at Dunn Echt.

conditions as affecting the apparent time of contact. With regard to the British expedition, great care has been taken to choose stations where the weather can be depended upon. But in cases where the method of duration is applied, the observations will be useless if there be not a very clear atmosphere both at ingress and at egress.

De l'Isle's method, on the other hand, requires a perfect observation only at the time of one of these phases. Hence the nations which have adopted this method are less likely to be disappointed than others.

GEORGE FORBES

(To be continued.)